Sachs's "Text-book of Botany," by Prof. E. R. Lankester, the Savilian Professor is also spoken of as having "discovered the sexuality of plants." It would interest students of the history of botany to know to what extent the writer of either of these articles is able to corroborate this statement by reference to Sir Thomas Millington's writings. In his recently published "History of Botany," Prof. Sachs gives the following account of this alleged discovery:—"In all histories relating to the subject of sexuality, a certain Sir Thomas Millington—otherwise unknown in the history of botany—is mentioned as deserving of the credit of having first indicated the stamens as the male sexual organs. The only information, however, which we have in support of this is contained in the following statement by Grew in his 'Anatomy of Plants,' 1682, p. 171, ch. 5, § 3:—'In conversation on this subject'—viz., on the part played by the stamens (termed by Grew the 'attire') in the formation of seeds—'with our learned Savilian Professor, Sir Thomas Millington, he gave it as his opinion that the 'attire' serves as the male organ for the production of the seed. I at once replied that I was of the same opinion, gave him some reasons for it, and answered some objections which might be made to it.'"[*] In the first edition of Grew's work, 1671, he attributes no sexual function to the stamens; but in the edition of 1681 he thus continues, in substance:—It appears firstly, that the "attire" serves to separate certain superfluous portions of the sap in order to prepare for the production of the seed. Just as the foliature (floral leaves) serves to carry away the volatile saline particles of sulphur, so the "attire" serves to diminish and adjust the atmospheric portions, in order that the seed may become more oily and its principles better fixed. The flowers have therefore usually a more powerful odour than the "attire," because the saline is stronger than the atmospheric sulphur, which is too subtle to affect the senses. An analogy drawn from the animal kingdom follows, which is hardly quotable; but Sachs points out how wonderfully any germ of truth in Grew's hypothesis was corrupted by the chemical theories and strivings after a false analogy of the day. It is difficult to see that there was really any advance in this hypothesis upon the state of knowledge in the time of Theophrastus (B.C. 371–286), who distinctly recognised some individual plants as male, others as female. Whatever merit also is due to Millington must, unless there is other record of his services, be at least equally shared with Grew.[†] It does not appear, however, that either of these botanists even attempted to confirm their conclusions by experiment. The merit of the first discovery of the true function of the stamens is assigned by Prof. Sachs to the German botanist Camerarius, in his "De sexu plantarum epistola," published in 1694. This tract closes with an ode, reminding one of Darwin's "Loves of the Plants," beginning thus—

> "Novi canamus regna Cupidinis,
> Novos amores, gaudia non prius
> Audita plantarum, latentes
> Igniculos, Veneremque miram."

ALFRED W. BENNETT

6, Park Village East, London, Nov. 29

The Late Eclipse

ON my return from India I should like to say a few words about some letters which appeared in the *English Mechanic* during my absence. Mr. Proctor, and a writer signing himself "A Fellow of the Royal Astronomical Society," comment in these letters on the result of the late Eclipse Expedition. It would be better if these discussions were postponed until the results are published by the Royal Society, but if writers who have not heard anything beyond a few short telegrams take it upon themselves to enlighten the public as to the value of photographs which they have not seen, a few remarks of one who has seen them become necessary.

If the telegrams written by me have given rise to the misunderstanding, I am sincerely sorry for it. I have had no personal interest either in the success or the failure of the expedition. The Royal Society has done me the honour to entrust me with the task of carrying out a programme sanctioned in detail by the Eclipse Committee. This I have done to the best of my ability, and in wording the telegrams in question I avoided, carefully any expression which might have raised expectations, not to be fulfilled on the arrival of the photographs. If the impression has been propagated that the expedition has not obtained any results of great importance, it is the fault of those who, thinking I had an interest in exaggerating the importance of the results, have taken away from the meaning of my words, which in reality remained far below the truth.

There cannot be the slightest doubt that the photographs obtained by the prismatic camera are full of interest and importance. They solve the question in which part of the spectrum the chief photographic rays of the corona are situated. They open out almost an entirely new field of inquiry, answering questions which could never have been answered by any other method, and suggesting new questions to be answered hereafter.

I should have liked to postpone the question whether it is possible to photograph in all its details the spectrum of the corona in the time available during eclipses, until Mr. Proctor's long-promised mathematical solution has appeared. As, however, we have had to wait for it already a considerable time, I venture to submit to your readers the following considerations:—The prismatic camera is a spectroscope without collimator. It has given us photographs after one minute's exposure, and would have done so in less time under more favourable atmospheric conditions. If we add a collimator and telescope to this camera, we shall have an arrangement similar to that which actually was employed for the photographs of the spectrum. If the lens of the telescope is, as regards diameter and focal length, like that of the camera; if, further, the focal length and diameter of the collimator lens is such that it would collect all the light which passed through the objective of the telescope, if the slit plate was removed, the only diminution the intensity of the light would be caused by the absorption through the two additional lenses and by the diffraction of the slit. The influence of diffraction can be reduced to a minimum by suitably altering the aperture of the collimator lens and by using a slit not too narrow. We should thus have an instrument capable of photographing the spectrum of the corona in one minute.

This is not the place to discuss whether the failure of the spectroscopic cameras was due to atmospheric causes, to the instruments employed, or finally, to my own fault. It will, I believe, be found hereafter, that the experience gained by even these failures will prove useful on future occasions.

In enumerating the results of the expedition the photographs of the corona and the sketch taken by the Hon. H. N. Shore ought not to be forgotten. The time observations were conducted with as much accuracy as the instruments permitted.

Sunnyside, Upper Avenue
Road, N.W., Nov. 20

ARTHUR SCHUSTER

Lommel's Optics

I AM indebted to Prof. W. N. Hartley for a correction in my review of Lommel's Optics, the proof-sheets of which did not reach me in time for revision. The translator of the work is evidently right in using the term napthalin red for the body which exhibits the fluorescent spectrum depicted in Fig. 6 in the article. The substance in question, Prof. Hartley states, is also called Magdala red, and has the elegant chemical name of Azotrinaphthyldiamine. I am also obliged to my friend Prof. H. M'Leod for pointing out that the mode of exhibiting the formation of the rainbow described by Prof. Lommel, is to be found in Jamin's "Cours de Physique" (tome 2, p. 782), although the substitution of a spherical flask filled with water for a solid glass globe, as described by Jamin, is more appropriate and convenient. Pouillet (tome 2, p. 769) also gives, I see, a somewhat similar experiment, using a cylindrical glass vessel filled with water.

W. F. B. ("the Reviewer of Lommel's Optics")

The Rainfall

IN NATURE, vol. xiii., p. 70, under the head of "The Rainfall," you allude to the extraordinary rainfall for 1875 in Great Britain, and call it a plague of rain; you further call attention to the astonishing fall of 1·287 inches for each of the three hours between 4 and 7 A.M. on the 1st of September last, at Sikawei, in China; and to the total quantity that fell there during the twenty-four hours that elapsed between 4 P.M. on August 31 and the same hour on the day following, viz. 8·59 inches.

I believe that a very heavy rainfall indeed was registered in South Devon in September last, the fall in one hour and in a total of twenty-four being unprecedented; but I have mislaid

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[*] I have not Grew's work at hand, and am therefore retranslating Sachs's translation.

[†] Grew was born in Coventry in 1628, and died in 1711; in 1677 he was appointed Secretary to the Royal Society.

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the record. It would be interesting to have this accurately stated, and more widely published.

At Bangalore, in the Mysore Province, I once registered an inch and a quarter in twenty minutes; and seven and eight, and more inches have been occasionally gauged during a heavy twenty-four hours' fall, notably so in 1856, when disastrous floods occurred. Somewhere about that period a most extraordinary fall of rain occurred at Madras. I regret that I have not the record by me, but I am right in saying that more than twenty-three inches fell in twenty-four hours! and that more than seven inches fell within six hours! This was gauged at the Madras Observatory, and registered every hour; the statement can therefore be easily corroborated by a reference to the superintendent at that place. A coffee planter on the western Ghauts of Mysore told me that at Hoolikul he had gauged, in August 1874, 13¾ inches in one day, and 10¼ the next. He described it as a sullen, intermittent, continuous downpour, the monotony of which was very depressing. At Mahableshwar, on the same line of Ghauts, the average fall is 240 inches, chiefly in the four or five months, from May to September inclusive; while at the Cherrapoonji Hills, not very far from Calcutta, the average fall is over 600 inches, or (say) seventeen yards of rain! My notes are in a book that I left in India, but I am within the mark in what has been stated above.

The meteorology of India would furnish many startling incidents. It has not hitherto been sufficiently attended to, or recorded, and much valuable time has been allowed (like the rainfall) to run by; but attention is now, I believe, being paid to its systematic registry, and to the publication of accurate results. I have some interesting records, however, of the Province of Mysore, which I would gladly place at your disposal should you desire to have them.

J. PUCKLE

A New Palmistry

IT is the old story—"In striving to be concise, I have become obscure." If Mr. Mott will refer to my abstract of Prof. Ecker's paper, he will find directions as to procedure, very briefly stated, I will grant; for I, and others too, more competent to judge than myself, had no idea that the subject would attract the attention which it has done. At Mr. Mott's desire, however, I give him a literal translation of Prof. Ecker's directions (*op. cit.* p. 73) in full:—

"With regard to the method of measurement on living individuals, I will merely remark that the hand must be simply laid, with the fingers closed together, upon a board or a piece of paper, upon which a well-defined perpendicular line has been drawn. With this latter the axis of the middle finger and its metacarpal bone is made precisely to coincide. Every lateral movement of the middle finger naturally alters also the position of the other fingers, and every movement of the fingers upon the metacarpal bones towards the pollex turns to the advantage of the position of the index, while that toward the little-finger-edge of the hand to the advantage of that of the ring-finger. The tips of the fingers (without nails *à la Chinois*) are then outlined with a pencil, halved longitudinally; its cut surface being applied against them."

JOHN C. GALTON

Nov. 29

I HAVE made a number of determinations of the relative lengths of the "index" and "ring" fingers of both hands, the results of which, I think, very decidedly show that there is a great dissimilarity between the two hands. The hands of twenty-two persons were examined; in ten there was similarity between the hands as regards the relative lengths of the two fingers in question; in twelve there was dissimilarity. In the case of eight out of the ten the "ring" was longer than the "index;" in one case the "index" was the longer, and in the remaining instance the two fingers were of equal length. Of the twelve cases which exhibited dissimilarity, six had the "ring" longer than the "index" in the left hand, and five in the right hand; four had the "index" longer than the "ring" in the left hand, and in three the same relation existed in the right hand.

M. M. PATTISON MUIR

The Owens College, Manchester, Nov. 27

Faye on the Laws of Storms

M. FAYE's paper on cyclones and waterspouts, of which you have lately published a full abstract, seems very unsatisfactory. The statement in NATURE, vol. xii. p. 401, of the laws of the cyclone's motion is no doubt true, but it is avowedly not original. But the succeeding parts, where the dynamics of the subject are treated of, cannot be sufficient—I think I may say cannot be sound—because they take no account of the very remarkable facts of the geographical distribution of cyclones. If M. Faye's theory were true and complete, cyclones ought to be equally common in all equatorial and tropical regions, except perhaps that they ought to be commonest in the hottest parts. So far is this from being the case, that they are strictly local phenomena. They are formed in the West Indian seas, but not in the South Atlantic; in the Indian Ocean, both north and south of the equator, but much oftener on the eastern than on the western side of India; and, I believe, off the coast of California, but not that of Peru. Their periodicity is equally remarkable. In the West Indian and in the Chinese seas they occur chiefly at the end of summer, but in the Bay of Bengal after the equinoxes.

All these facts point to the origin of the cyclone, not, as M. Faye seems to think, in eddies formed in the upper currents of the atmosphere—how could eddies be formed in currents so totally free from obstructions?—but in eddies formed by the meeting and conflict of the two trade-winds where one of them is drawn across the equator. This hypothesis agrees with observation, and harmonises all the geographical facts relating to cyclones.

This simple and true theory is stated in a paper on the Law of Storms by Prof. Maury, in NATURE of June 12, 19, and 26, 1873. It had previously been stated for the Bay of Bengal, as the result of an examination of particular storms, by Mr. Meldrum, in a paper read before the Meteorological Society of Mauritius, and reported in NATURE, vol. ii. p. 151; and a letter of mine in NATURE, vol. iv. p. 305, maintained the probability of all cyclones so originating.

I think M. Faye is as unsatisfactory on waterspouts as on cyclones. I hope to follow this by a letter on waterspouts.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Nov. 16

OUR ASTRONOMICAL COLUMN

SATELLITES OF URANUS.—There are many amateurs in this country who possess instruments quite competent to show the two larger or exterior satellites of the planet Uranus. With the view to facilitate the identification of these objects, their angles of position and distances from the centre of Uranus are given below for 14h. Greenwich mean time for the last ten days of the present year, with the intention of continuing them while the planet is most favourably placed for observation as regards position and distance from the moon's place. They are deduced from the very convenient tables appended by Prof. Newcomb to his discussion of the observations of the satellites with the 26 inch equatorial at Washington, forming Appendix I. to the Washington Observations for 1873:—

At 14h. G M.T.		°	Titania.	"			°	Oberon.	"
Dec. 19 ...	Pos.	48·0	Dist.	20·8	...	Pos.	64·2	Dist.	23·1
20 ...	,,	18·3	,,	32·3	...	,,	33·8	,,	34·6
21 ...	,,	359·6	,,	32·1	...	,,	18·1	,,	43·4
22 ...	,,	329·2	,,	20·5	...	,,	6·1	,,	45·3
23 ...	,,	249·6	,,	16·6	...	,,	352·9	,,	39·4
24 ...	,,	206·1	,,	29·3	...	,,	331·4	,,	28·3
25 ...	,,	186·4	,,	33·9	...	,,	285·2	,,	20·0
26 ..	,,	163·4	,,	25·4	...	,,	233·4	,,	26·1
27 ...	,,	100·8	,,	14·9	...	,,	208·8	,,	37·6
28 ...	,,	36·2	,,	25·0	...	,,	194·7	,,	44·9
29 ..	,,	12 9	,,	33·9	...	,,	182·9	,,	44·6
30 ...	,,	353·2	,,	29·7	...	,,	168·4	,,	36·9
31 ...	,,	311·3	,,	17·0	...	,,	142·2	,,	25·5

The above angles are reckoned as is usual in measures of double stars, *i.e.*, from the N. point round by the east. The apparent diameter of Uranus by the Malta determination of Lassell and Marth will be 3"·87 on December 25th. With this value, should it be found more convenient, the arc values may be reduced to distances in diameters of the planet.

THE MINOR PLANETS.—No. 156 is announced as having been discovered by Herr Palisa, at Pola, on Nov. 22, in R.A. 2h. 54m., and N.P.D. 70° 23': it is of

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